



Mathematical psychology: Prospects for the 21st century: A guest editorial[☆]

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ABSTRACT

The twenty-first century is certainly in progress by now, but hardly well underway. Therefore, I will take that modest elasticity in concept as a frame for this essay. This frame will serve as background for some of my hopes and gripes about contemporary psychology and mathematical psychology's place therein. It will also act as platform for earnest, if wistful thoughts about what might have (and perhaps can still) aid us in forwarding our agenda and what I see as some of the promising avenues for the future. I loosely structure the essay into a section about mathematical psychology in the context of psychology at large and then a section devoted to prospects within mathematical psychology proper. The essay can perhaps be considered as in a similar spirit, although differing in content, to previous editorial-like reviews of general or specific aspects of mathematical psychology such as [Estes, W. K. (1975). Some targets for mathematical psychology. *Journal of Mathematical Psychology*, 12, 263–282; Falmagne, J. C. (2005). Mathematical psychology: A perspective. *Journal of Mathematical Psychology*, 49, 436–439; Luce, R. D. (1997). Several unresolved conceptual problems of mathematical psychology. *Journal of Mathematical Psychology*, 41, 79–87] that have appeared in this journal.

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1. Psychology and mathematical psychology therein

1.1. A glimpse of recent history

Psychology is a young science as opposed to a young field

[☆] A word is in order concerning the scope and style of this essay. There are many themes that could be selected for such a piece. The topic I have chosen centers on the health and future of mathematical psychology. It does include a necessarily very succinct history and a number of bordering issues along with some suggestions for remediation and improvement. Due to space limitations, I have had to limit mention and citations of a plethora of important names outside those of a very few of the pioneers in the field. A vast number of important investigators especially if they're on the younger side are neglected. In particular, I had to virtually entirely omit the contributions, many overlapping with mathematical psychology, stemming from the field of psychometrics. The same goes for a voluminous set of researches on categorization. I regret and apologize for these necessary omissions. And, though an individual may have performed valuable research in several areas of mathematical psychology, I refrained from making these multiple mentions. I should also mention that I cannot guarantee that all citations within a set of references in the same location in the essay will be homogeneous (e.g., all reviews of their work, all at about the same date, etc.). It was already laborious simply collecting these as they are. I also take license to occasionally mention important names sans specific citation. The style is meant to be informal, even conversational, so some notable individuals receive mention without specific citation.

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of discourse. Ponderings about psychological topics, casual and systematic, recede into recorded history. Yet we all know that scientific psychology is little more than one-hundred twenty-five years old or so, dating by convention from 1879, the year of establishment of Wundt's famous laboratory in Leipzig; this according to Boring's (1957) calendar of experimental psychology.

Without question, we have made rather startling progress by almost any measure, in that scant time. This progress has arguably accelerated since the 1940s due to the amazing new tools, so attractive and appropriate for non-physical sciences, proffered by von Neumann, Wiener, Shannon, and others.² I speak, of course, of automata theory (von Neumann), utility theory (von Neumann & Morgenstern), cybernetics/feedback control theory (Wiener), and information theory (Shannon) (see, e.g., Townsend and Kadlec (1990)).

A significant paradigm shift in psychology occurred when it began to move away from the grand and perhaps overweening schools toward less capacious, but more articulated venues containing more testable models and hypotheses. The grand schools were highly conspicuous in abnormal psychology, especially in the

² In all cases of name listing, there is no significance to their order.

various off-shoots of Freudian theory: for instance, Adler, Fromm, Jung, Horney, and Reich, to name a few. But experimental psychology also possessed broadly encompassing theories identified with individuals, as witness Thorndike, Lewin (also featured in social psychology), Skinner, Tolman, Hull, and Guthrie.

The 1950s and then the 1960s saw such quantitative areas as signal detection theory, mathematical learning theory, and foundational measurement theory aid and abet this transition to a less global but more rigorous, science.³ Names we think of in the signal detection connection are Peterson, Birdsall, Fox (engineers), Swets, Tanner, Green and Egan. Pioneers in mathematical learning theory include Bush, Mosteller, Estes and again rather quickly, Suppes, Atkinson, Bower, and Crothers, and Murdock.

In addition, just about the same time, psychologists and friends began to work out new theories of measurement that included structures more appropriate for the behavioral and biological sciences than had the 'classical' approaches of Campbell and others. We note well-known contributors like Suppes, Krantz, Luce, Tversky, and in somewhat different vein, Coombs, and N. Anderson. These investigators also overlapped the growing field of decision theory oriented toward actual human behavior. Other notable names around the same time, in allied pursuits with these various topics were Restle, Greeno, LaBerge, Sternberg, and Theios. Garner and Attneave and Laming helped bring information theory into the fold of quantitative psychology. And, the rather neglected (admittedly not quite so much in psychometrics), but fundamental topic of geometry in psychology was given a considerable boost by Shepard, Kruskal and others.

With the foregoing eye-blink history as preface I move to a list of woes that afflict psychology, afflictions that can be somewhat ameliorated by the practice of mathematical psychology.

1.2. *The tortoise and the hare*

It can prove a frustrating experience to compare psychology's pace of advance with progress in the 'hard' sciences. Except perhaps in the flurry of studies one may witness on discovery of a dramatic 'effect' (see discussion immediately below), steps in filling in data about a phenomenon not to mention testing of major theoretical issues and models, seem to occur with all the urgency of a glacier. One may wait years, before a modeler picks up the scent of an intriguing theoretical problem and carries it ahead. It is disheartening to contrast our situation with, say, that of microbiology. Perhaps the influx of quantitatively prepared 'outsiders' (look for later discussion) will help speed things up.

1.3. *The eternal pursuit of effects*

One of the 'blessings and curses' of modern psychology is the everlasting quest for 'effects.' Some of our most famous investigators made their name by discovery of a novel and enchanting effect. In some cases, these have led to a rich set

of phenomena and interesting, if rarely conclusive, explanatory models or theories. A downside is that the careful and steady, incremental growth of the science can be neglected, since the obvious rewards, or at least the 'grand prizes' in the field are accorded the discoveries of effects. But the interpretation of the initial effect that stands unaltered in the face of replication and parameter variation is quite atypical. Often, follow-up experiments require elaboration or complexification of the original rationale and frequently, yet more experimentation. Thus, this approach can be productive if carefully pursued experimentally and theoretically. Otherwise, our priorities can be decidedly skewed toward unearthing of new effects.

1.4. *Another portentous trade-off approach: Operationalism*

A close cousin of the 'effects' bias is the embodiment of one or more theoretical notions in an experimental paradigm, or set of paradigms. The growth of 'operationalism' was associated with the philosophical movement of logical positivism and the Vienna Circle (think Schick, Ayer, Carnap, Neurath, Hempel and influencers and fellow travelers such as Russell, Wittgenstein, Popper) and found a vent in psychology through efforts of Meehl & MacQuordale and the physicist Bergmann (it has been said that Bergmann influenced the social sciences far more than he did physics). Again, this approach possesses benefits, and aided psychology in shaking off the residue of impossible-to-answer philosophical conundrums that still adhered to the field in the early twentieth century. The loved and hated school of behaviorism came into existence and dominated experimental psychology for several decades.

In any event, there were snags, snags which were probably unanticipated by the founders of the 'operational definition' wherein philosophical claptrap was avoided by defining theoretical entities by way of the empirical operations through which observations were recorded. A substantial snag is the risk of circularity where a theoretical hypothesis points to an experimental result and vice versa. The theoretical and phenomenal restrictions are evident. Or, if as sometimes occurs, there are several more-or-less distinct operations relating to a phenomenon and presumed theoretical concept, these may turn out to be closely related (identical occasionally, this is good), unrelated, or even contradictory, depending on a subsequent theory developed to encompass the paradigms associated with respective operational definitions.

Mathematical psychology serves as a stiff antidote to the afflictions of 'effect-philia' and cul-de-sacs of overindulged operationalism. The necessity of providing a rigorous, economical, accounting of concepts and empirical through a quantitative model clearly combats the overly particular, and acts not only to accommodate an entire set of phenomena, but assays the ability of diverse theoretical notions and experimental operations to 'live together' within the same theory.

1.5. *Anti-replication and anti-null effects bias*

With regard to afflictions in psychology (and probably a number of other sciences), I mention two which are not particularly solvable through modeling, but I want to get them off my chest: One is the heavy bias against replication. Too few studies are published that precisely replicate an earlier study. In fact, younger investigators in particular, are warned to include variations so that editors and reviewers will not reject their 'replication' out of hand. This is not an earmark of a mature science. Long ago, William K. Estes spoke of this regrettable prejudice in a seminar during my graduate student days. It seems just as true now as it did then.

A cognate weakness that many writers have remarked on, using a variety of terms, is the bias against publication (or even consideration for publication) of 'null' results. How on earth can

³ Somewhat truthfully and perhaps somewhat waggishly, the period of the 1960s at Stanford has been referred to, at least by the erstwhile graduate students of the times, as "the golden days of mathematical psychology". Frequent workshops with other math-psych bastions such as University of Michigan and University of Pennsylvania were held, the milieu was outstanding with a modeling faculty that included William Estes, Richard Atkinson, Gordon Bower and Patrick Suppes. Many of the students during that period were to become leaders in various facets of psychology. The pack in my cohort includes Michael Humphreys, Richard Shiffrin, Don Hintzman, Robert Bjork, Stephen Link, Chiziko Izawa, William Batchelder, Donald Horst, Jack Yellott, Joseph Young, David Rumelhart, and Michael Levine. In addition, many already or soon-to-become, notables visited in a post-doctoral or visiting scientist capacity. With no apologies for the nostalgia, it was a fabulous time and place to be entering scientific psychology.

psychology advance when only ‘positive’ findings with regard to a hypothesis, model, etc., are made available to the scientific public? A number of suggestions have been put forth to circumvent this obstacle, including an archive of negative findings, perhaps sorted according to topic. To my knowledge, this strategy has never been seriously attempted. With the current revolution in electronic publishing, it might be worth a try. With the increasing ability of information systems to implement content-addressable search, even just the compilation of frequencies of positive vs. negative findings could be implemented in the drawing of inferences.

Aside from null results, recent years have seen a lively debate about the value of null hypothesis testing, in addition to various means of improving the strategy and avoiding its threatening sloughs of despond.

1.6. Methodology = Modeling plus statistics and psychometrics

Another associated topic involves methodology in psychology. A multitude of writers have decried the methodological fetters accompanying decades of over-reliance on standard statistics, statistics originally intended more for assessing grain production under varying conditions than for an evolving systematic science. On the one hand, traditional statistics and its offspring have served as effective tools for much of the advancement of psychological science in the last century. On the other, an argument can be made that we have become much too dependent on its fruits in ways that have succored the tendency to ‘live-with’ loose, verbal theories. Done right, the field of mathematical psychology offers a prescription for using quantitative theory to impel theory-driven methodology.

Of course, there will always be a need for statistics. In addition to testing various traditional hypotheses, providing for confidence intervals, etc., it is highly useful to have in hand tools to test model fits and, even better, to compare models against one another (see, e.g., the useful special issues of *Journal Mathematical Psychology* (Myung, Forster, & Browne, 2000; Wagenmakers & Waldorp, 2006)). This topic will reappear in later sections of the present essay.

1.7. A regrettable neglect of proper scientific referencing

I will conclude this section with a little belly aching pertinent to the behavioral sciences in general and psychology in particular. Obviously, we all know of areas in psychology that are amenable to modeling and could clearly profit from it. Often, verbally based “theories” may be underpinned (or even contradicted) through previously published quantitative research but the verbal theorist fails to use or cite it. It may well be that the ‘errant’ investigator thought up the approach independently in a verbal and qualitative way, as psychologists often do. Or it may be that they simply don’t feel that anything accomplished mathematically has to be cited by non-modelers, as noted above. In either case, it is bad science and insulting to the theorists and methodologists who labor mightily to advance psychology as a rigorous science. I trust that a physicist would not fail to recognize mathematical results that formed a sturdy basis under her theory or tools.⁴

A related gripe concerns the tendency of investigators in different quantitative, but overlapping, disciplines to ignore work in the areas of their research ‘cousins’. Sometimes this simply takes the form of failing to utilize helpful material from other research domains, but not infrequently, it may involve lapses in

citing even mathematical theorems that have been previously published elsewhere. Mathematical psychology is occasionally prey to this tendency since we overlap such a broad sweep of mathematically oriented research venues. The writer has witnessed several examples of these oversights. Economics, industrial engineering, operations research, biophysics, many areas of artificial intelligence (robotics, pattern recognition, problem solving ...) clearly provide for rich interaction among investigators but are likewise prone to this type of abuse.

2. Mathematical psychology: Ruminations on its evolution and position

2.1. Continued development of areas of mathematical psychology and rumors of a demise

As observed earlier, mathematical psychology emerged from embryo in the late fifties and early sixties of the twentieth century. Already by the end of the sixties, some were pronouncing the demise of mathematical psychology.⁵ Did it really die, despite the ostensible continued existence of practitioners of that field? If not an obvious corpse, is it in dire peril?

Let’s pause a moment to espy the fate of some of the major areas of early effort in mathematical psychology mentioned earlier. Perhaps they may shed some light on this assertion. In addition, this little tour will allow us to discern where major branches of the field have themselves perambulated.

2.2. Foundational measurement

Several of the founders’ names were mentioned earlier but it would be remiss not to cite several major seminal works in this area: The three foundational volumes of Krantz, Luce, Suppes, and Tversky (1971), Luce, Krantz, Suppes, and Tversky (1990a,b) and Suppes, Krantz, Luce, and Tversky (1989), the excellent pedagogical text of Roberts (1979), and the seminal topologically based volume of Pfanzagl (1968).

Foundational measurement theory has continued to attract highly skilled mathematicians and quantitatively oriented psychologists from throughout the world and to advance knowledge, especially on the critical topic of ‘meaningfulness’. Technical discussion is beyond my scope here, but informally, ‘meaningfulness’ relates jointly to how a measurement scale represents qualitative aspects of the real world, and the degrees and types of invariance that properties of a scale enjoy under permitted transformations of that scale (e.g., Krantz et al. (1971) and Roberts (1979); for a recent thoughtful statement, see Narens (2003)).

One impediment to more usage of foundational measurement theory has undoubtedly been the relative paucity of effort and results on an ‘error’ theory which could provide a ready implementation of statistical procedures with data. Groundbreaking work continues on this challenge (for recent progress on stochastic approaches to foundational measurement which subsume the traditional error theory, see, e.g., Niederée and Heyer (1997) and Regenwetter and Marley (2001)).

I believe this field is and will continue to be, of interest not only to the behavioral and biological sciences, but also to philosophy of science and epistemology in physics, although at present the substrata primarily relate to Newtonian rather than relativistic physics. At any rate, this branch of mathematical psychology is apparently not responsible for the reputed passing of the field.

⁴ I suspect a number of readers can think of instances of this phenomenon, perhaps intersecting their own work. I rather cravenly fail to mention names in order to avoid litigation and other unpleasant retribution.

⁵ As far as I can ascertain, these announcements have been confined to verbal remarks. However, they have occasionally been uttered by renowned psychologists.

2.3. Signal detection theory

Signal detection theory, as most readers of this essay will know, emerged in the 1950s as a confluence of Neyman/Pearson statistical decision theory and ideal detector theory of electrical engineering (e.g., one of my early favorites is Peterson, Birdsall, and Fox (1954)). In psychology, the Green and Swets (1966) book has become a classic with other volumes such as Egan (1975) following up on interesting byways. Early on, there were clear and continuing associations to be made with Thurstone's popular discriminational processes theory.

Signal detection theory went through a period during the sixties and early seventies in which psychologists proffered a number of models that lay outside the engineer-oriented mathematical communication theory (as exemplified by the central content of the popular (Green & Swets, 1966)), the latter also incorporating (but was not limited to) elementary statistical decision theory. In addition to Luce's adaptation of his choice theory to detection and recognition situations (e.g., Luce (1959, 1963a)), a number of finite state models were put forth and evaluated (e.g., Atkinson and Kinchla (1965), Krantz (1969) and Luce (1963b)). Although these contained interesting capabilities, not always captured by the dominant theory (as exemplified by Green and Swets (1966)), especially learning effects (e.g., Kinchla, Townsend, Yellott, and Atkinson (1966) and Luce (1959)), they currently see only occasional employment.

In contrast, the dominant theory is now ubiquitous in psychology in general as well as in sensory sciences, usually employing the ubiquitous Gaussian distributions (nonetheless, two-state models vie with the continuous theory in certain areas; see the Wixted reference below and citations therein). With regard to the latter characteristic, it would seem that certain foundational results could be beneficially employed by experimental researchers (e.g., Marley (1971)). For example, ideal detector theory is flourishing as are multidimensional theories of signal detection. An example of the latter is general recognition theory, which was originally developed in order to study interdimensional interactions (e.g., Ashby and Townsend (1986), Kadlec and Townsend (1992) and Thomas (1999, 2003)). One of the major contributions of signal detection theory was the centrality of decision mechanisms, even in putative "purely sensory" domains. This facet continues to offer fresh perspectives as in the Wenger and Ingvalson (2002) study which found a powerful role of the decision process in the perception of holistic vs. non-holistic object perception. It has been widely employed as a theory of categorization (e.g., Ashby and Maddox (1990)); many of the models in Ashby (1992) are signal detection based).

Certainly, signal detection theory should be studied (usually it is covered rather cursorily, if at all, and in a non-quantitative fashion—more is the pity; see below) even by students in their introductory courses. It stands as the prototypical theory-driven methodology, since it can be employed to discern decision and learning bias from 'true' sensory or sensitivity (e.g., signal-to-noise ratio) effects in such diverse fields as hypnotic phenomena, to trial-witness memory, to laboratory psychophysics or learning and cognition experiments e.g., Swets (1996); see Balakrishnan (1999) for an alternative method of analyzing sensory and bias effects). For instance, Wixted (2007) argues for a 'traditional' type of signal detection model against less mathematized but process oriented, two-process kinds of models, in certain areas of cognition (e.g., see Balota, Burgess, Cortese, and Adams (2002), Diana, Reder, Arndt, and Park (2006), Heathcote (2003), Hockley and Cristi (1996), Malmberg, Zeelenberg, and Shiffrin (2004) and Yonelinas (1994)). MacMillan and Creelman (2005) compile a worthy set of signal detection-based methodologies provided in a tutorial style. Wickens (2002) provides a basic introduction to many of the fundamental concepts.

In any event, there is no reason to think that signal detection theory encouraged anyone to proclaim the decease of mathematical psychology.

2.4. Decision theory

What about the field of decision making? One branch of effort primarily theoretical but impelled partly by a growing literature of experimentation, finds its roots in the axiomatic foundations laid down by von Neumann and Morgenstern (1953). Psychologists dedicated to this tradition consisted partly of those also contributing to foundational measurement, and indeed, often similar tools are found in their theoretical arsenal (e.g., Luce, Suppes, Krantz, and Tversky). Of course, this branch also included those with statistics or economics backgrounds (e.g., Savage).

A massive and influential development in the field came about through the efforts of Tversky and Kahneman, who discovered a number of human choice situations in which people veer drastically away from the classic (and even some newer) axiomatic theories. Certain of these cases flow from their theoretical results, especially prospect theory (Kahneman & Tversky, 1979). As everyone should now know, this corpus of work earned Kahneman the Nobel Prize in economics and would undoubtedly been awarded simultaneously to Tversky were it not for his untimely passing.

The field of decision making has historically been somewhat separated into a set of quantitative theorists (mostly in the axiomatic or statistical framework) and a set of experimentalists, the latter largely made up of psychologists. Of course, there still are many who do both (e.g., One subdivision of the experimental group has inclined toward testing predictions made by the axiomatic or statistically-based models (Birnbaum, 2004). Another area has concentrated on continuing the quest for psychological behavior that seems at odds with various facets of the axiomatic theories [especially utility theory]). Yet another has evolved non-quantitative models or theories that attempt to be heavily real-world oriented (e.g., how do people make decisions in a group-crisis?), and experimented thereon.

Although various facets and extensions of utility theory per se are still active research areas, a number of investigators have moved on to explore the preferential laws governing risk (Weber, Shafir, & Blais, 2004). In addition, the perhaps overdue appearance of hedonics in the consequences of decisions has occurred (Mellers, 2000). Gigerenzer and colleagues have been a powerful voice in plumping for models based on all-too-human limitations in processing capacity and sometimes rationality (e.g., Gigerenzer and Todd (1999)). Wishing to avoid charges of excessive modesty, I hasten to mention work that seeks to be in the spirit of strong quantitative theory but heavily invested in psychological and biologically flavored knowledge (Busemeyer & Townsend, 1993).⁶ In any event, I can locate little in the evolving field of decision making that should have precipitated augurs a terminal malady of mathematical psychology.

2.5. Psychophysics

I somewhat artificially separated signal detection from psychophysics and it might legitimately be argued that neither is strictly a subfield of mathematical psychology. Moreover, due to space and 'psychological distance' concerns, I must neglect the now sizeable field of sensory sciences, which of course, heavily overlaps both psychophysics and signal detection (and uses methods

⁶ I refrain from mentioning the likelihood of a pummeling about my head and shoulders by my colleague, Jerome Busemeyer.

from both). Nonetheless, I think it is fair to say that in some senses the psychophysics of Weber, Fechner (to a lesser extent Wundt), Helmholtz, and others was a progenitor, along with statistics, of mathematical psychology. Also, modern mathematical psychologists and psychophysicists have long been attracted to it, applying concepts from measurement theory and functional analysis (e.g., see Falmagne (1985) and Luce, Bush, and Galanter (1963)) as well as more process models (e.g., Baird (1997) and Link (1992)).

From some viewpoints, Stevens (1951) should probably be considered as the twentieth century heir of classical psychophysics, through his innovation of the necessity for a hierarchy of measurement scale types as well as his new experimental methods of scaling, the so-called direct methods. One influential investigator whose work could be listed in several of the present categories including the present is that of Shepard (1964). Another is Anderson (1981). A substantial and evolving theory of psychophysics with roots in Fechner's original developments is found in the work of Dzhafarov and Colonius (e.g., Dzhafarov (2002) and Dzhafarov and Colonius (2007)). Nosofsky has contributed an influential modeling approach which links up psychophysics, psychometrics (via multidimensional scaling), and information processing (e.g., Nosofsky (1984)).

An enduring challenge, not unrelated to the issue of multidimensional interactions mentioned earlier, is the inclusion of context effects in psychophysical scaling (see, e.g., Helson (1964) and Parducci (1956)). The drawing together of the psychophysical approaches with process model approaches to context and interactions has begun (e.g., Chapter 16 in Baird (1997), Hughes and Townsend (1998) and Link (1992)) but is likely still in its infancy. Nonetheless, it appears that such a synthesis can be useful. For instance, a coalition of psychophysical with process methodology has elucidated aspects and challenges of the Stroop effect not previously visible (Melara & Algom, 2003). Sarris has made fundamental contributions to relational psychophysics in comparative-developmental psychology (e.g., Sarris (2006)).

2.6. Neural modeling

One very significant player in quantitative theory in psychology as well as cognitive science has been neuropsychological modeling. It might have been expected to provide a major counter weight to some of the less propitious forces facing mathematical psychology. Due to the potentially symbiotic linkages that could result when both are applied in the spirit of reductionism (but see Uttal (1998)), plus its almost startling resurgence in recent years, I take a little more space on this topic.

Like some of the other branches of research, an adequate history of neural modeling would take up at least one volume. However, we can limn in some of the most evident sign-posts. Although neural modeling was certainly around at the time that mathematical psychology received its formal impetus, and there have been significant intersections over the years, it is not routinely thought of as a sub-field of mathematical psychology.

Modern history of neural modeling goes back at least to Rashevsky and Ashby in the 1940s. The fifties saw emergence of logic network based thinking (flowing undoubtedly from automata theory of von Neumann, Turing, and others) by McCulloch and Pitt. The ultimate influence of Hebb's well-known principles of synaptic learning, though not so rigorously presented originally as some of the other candidates for attention, would be difficult to overemphasize.

At the juncture where mathematical learning theory began to fade in popularity (no causal implication intended here, merely a time-marker), Grossberg (1969) began to publish his first neural modeling work which, incidentally, included an emphasis on learning and motivation. That work was, and is, founded on

non-linear (typically multiplicative) differential equations, most often with strong systems-oriented interpretations afforded the equational elements.

About this time too, two investigators in the cognitive science revolution, Minsky and Papert (1969), published a book innocently entitled "Perceptrons". As most readers of this essay are well aware, perceptrons are basically linear, and usually deterministic, pattern recognizers; members of the class of systems known as linear discriminant classifiers. These authors showed in a carefully reasoned treatise, that perceptrons are incapable of seemingly quite elementary topological distinctions.

Needless to say, their exegesis did not have the effect of furthering the general interest in perceptron theory or perhaps, even an encouragement with regard to neural modeling per se. In fact, some have felt that it had a rather devastating consequence on activity in neural modeling.

During the years that interest in neural modeling paled, Grossberg and a few other theorists, such as James Anderson, kept the fires aflame (Anderson, 1995). Anderson's models could be viewed as more sophisticated upgrades (e.g., incorporating stochastic noise, Hebbian learning devices, and non-linear decision structures) of perceptronic tenets.⁷

Then, in the 1980s, hurtled the connectionistic meteor, driven in substantial part by the labors of Rumelhart and McClelland (1986). Interestingly, Rumelhart had been a graduate student in the Stanford mathematical psychology training program in the 1960s. Even though some specialists would prefer to offer distinct definitions for "connectionism" vs. say, "distributed processing", they are often used interchangeably to indicate what we might call neuralistic modeling. "Neuralistic" might be a reasonable neologism for this field since practitioners strive to let neurophysiology and neuro-anatomy guide their efforts while benignly neglecting less critical aspects of these disciplines. They are occasionally taken to task for transgressions in this regard, but theorization has always been a matter of emphasizing what seems most vital and ignoring the rest. Certainly such criticisms apply to all but the most microscopic models of neural functioning; and the latter are often of little interest to psychologists. It is also worth remarking, given our earlier discussion, that learning theory, always a key ingredient of neural modeling, made an impressive come back in the wake of the renaissance of the latter.

Although endeavors were made by those involved with the Society for Mathematical Psychology to encourage affiliations and interactions (e.g., by appointing connectionistic associate editors to *Journal of Mathematical Psychology*; inviting keynote addresses by leaders in connectionistic modeling, etc.), and although many mathematical psychologists have labored from this perspective, the attempts to embrace this field perhaps have not been entirely successful. There are notable exceptions, including the work of Kruschke (1992).

Neuropsychological modeling has certainly waxed and waned over the past half century or so, and has proceeded more or less independently of mathematical psychology, but it doesn't appear to have been responsible for the obituary of the latter at any point in time.

2.7. Information processing approach

Like the other topics discussed here, the information processing approach possesses somewhat fuzzy boundaries, but perhaps

⁷ In the discipline of neural modeling even more than others, space and the natural emphasis of this essay prohibit listing of a sizeable set of investigators primarily associated with other fields, which have made fundamental contributions to neuro-psychological quantitative theorizing.

even more so. A central tenet seems to be the representation of perceptual, cognitive, and/or motoric mechanisms, usually confined to a certain task setting, via a set of subsystems with the information flow (itself usually a fuzzy concept) depicted via the so-called, and ubiquitous, flow diagram. Lachman, Lachman, and Butterfield (1979) provided what I regard as a quintessential tutorial on the information processing approach, and one of the last rigorous treatments of cognitive psychology for undergraduates. Of the early textbooks in mathematical psychology, probably Laming (1968) was the closest to the information processing approach. Undoubtedly spawned by computer science (e.g., automata theory), and aided and abetted by information theory and cybernetics, the generic conception was picked up just as fast by general experimental psychologists as by mathematical modelers, sans the rigor of the latter. Yet, the seminal efforts of Sternberg, Sperling, Estes, Falmagne, Atkinson, and others, helped attract modelers intrigued by the idea of analyzing a system down into its functional components, even if (or *because*) the properties of the system *in action* might reveal emergent properties. Later contributors of the modeling ilk include Nosofsky, Massaro, Link, Yellott, Ratcliff, D. Meyer, Colonius, Diederich, J. Miller, Vorberg, Bundesen, E. A. C. Thomas, Massaro, Logan, Schweickert, Dzhaferov and others.

Models of response time have been especially influential and productive in uncovering underlying processing mechanisms. The most popular types of response time models, often including accuracy predictions have been those based on random walks (e.g., Link and Heath (1975)), diffusion processes (e.g., Ratcliff (1978) and Busemeyer and Townsend (1993)), and counting processes (e.g., Smith and Van Zandt (2000)).

I believe the essence of the information processing approach provided a milieu that helped prompt investigations into issues of model mimicking. This influence was even felt in modeling of learning and memory. Thus, Greeno and Steiner (1964) analyzed model equivalence within Markov chain models of memory. Batchelder (1970) attacked what seemed to be a mimicking issue regarding incremental vs. all-or-none learning and showed how to distinguish them. My own efforts on parallel vs. serial model testing began shortly thereafter (e.g., Townsend (1969, 1971, 1972) and Townsend and Wenger (2004)). Of course, knowledge of when and how models *cannot* be differentiated can assist in erecting a meta-theory or methodology that is *capable* of such assay (e.g., Townsend (1976a,b)). In my not-unbiased view, the information processing approach is still alive and prospering, although perhaps not always under that rubric.

2.8. Mathematical learning theory

And then there is that other early pillar of mathematical psychology, mathematical learning theory. The germinating work of Bush and Mosteller (1955) and Estes (1950) gave rise to a decade or so of fervent activity on mathematical models of learning, including a wave of anthologies and monographs (e.g., Bush and Mosteller (1955); later, Atkinson, Bower, and Crothers (1965); very late, Restle and Greeno (1970)).

Even during the 1960s, mathematical learning theory was beginning to move toward more emphasis on memory and less on learning. By the mid-seventies publications of theoretical and experimental effort devoted to mathematical learning theory had diminished precipitously. Furthermore, it could be (and was, at least informally) argued that finite-state Markov learning models, a mainstay in the field, were beginning to appear rather baroque, sometimes without convincing signals from the data that such elaborations were required. Perhaps before self-corrective measures could eventuate, other forces essentially moved in to occupy the territory.

Thus, by this time, the budding field of cognitive science, with both its theoretical content as well as implementation heavily determined by digital computers and automata theory, was increasing enormously in popularity, with Simon and Newell perhaps leading the charge in areas close to experimental psychology. Contributions began pouring in not only from experimental psychologists but also philosophers, computer scientists, electrical engineers and applied physicists. Such innovations as production systems soon provided a powerful lure to psychologists seeking a richer milieu for concepts about mental operations. Likely, it is primarily the confluence of cognitive science as a new and promising field along with the perceived failure of mathematical learning theory to ‘pay off’ that caused a substantial hiatus, if not termination, of the latter. Into the bargain, many of the founders of mathematical learning theory were increasingly attracted to regions of study more closely allied with cognitive science, such as the broad approach of human information processing. It seems fair to say that the well-known Atkinson and Shiffrin model of short-term memory and control processes (Atkinson & Shiffrin, 1968) epitomized this movement. The subsequent launching of the ACT models by J. R. Anderson and colleagues (e.g., Anderson and Bower (1973)) in some ways intersect both the information processing approach and the emerging cognitive science paradigm (e.g., a la Simon & Newell).

The evidence seems to point to the recession of mathematical learning theory in the late 1960s as one key marker that convinced some investigators of the weak health, if not demise, of mathematical psychology. Moreover, a number of modelers who cut their teeth with the ‘classical’ learning models, were now moving wholesale into ever broader and more rigorous models of memory. I refer in particular, to the works of Hintzman (1986), Izawa (1971), Murdock (1982), (e.g., Raaijmakers and Shiffrin (1981)), (e.g., Humphreys, Bain, and Pike (1989)), which investigators have over the past few decades made this field into a poster child of how behavioral research should be carried out.

Okay, so we could lay some of the blame for the perhaps premature rumor of the death (but not interment!) of mathematical psychology on the adventitious events pertaining to learning theory in the late 1960s. However, it has always seemed paradoxical to me how learning per se was shoved into the corner for several decades, even though memory became superseded as a legitimate topic in cognitive science and mathematical psychology per se. How did these memories originally become instated? With few exceptions only in the relatively lonely (for awhile!) terrain of neural modeling (e.g., Anderson (1973) and Grossberg (1969)); and shortly with a resurgence from the Anderson quarter e.g., Anderson (1990), did learning theory persevere. While it is fair to say that learning played some part in some of the burgeoning memory models, it was at best a minor role. Learning as a legitimate research topic is happily now back with us big time.

In any event, it is time to abandon this historical, if somewhat whimsical, excursion and pose the query as to the state of health of the field today.

2.9. Trends, education, politics, and tenure

One can attempt answers along many dimensions. First, the ‘glass is half full’ perspective: The Society of Mathematical Psychology continues to serve many functions that are conducive not only to the highest standards of research in mathematical psychology, but to encourage the entering of young scientists into our field. Its members contribute expert reviewership to a broad spectrum of research areas vis-à-vis journals and scientific grant proposals. They provide some of the top research, especially through model and theory building, in scientific psychology. Many regularly bring in valuable grant resources to their universities,

even in these extreme financially harsh times. Many members of the Society have served on strategic committees and panels in national institutes and other societies and federations.

Nonetheless, there are serious “glass is half empty” concerns: For instance, with reference to membership of Society of Mathematical Psychology, we find that unfortunately, numbers have, with some fluctuation, tended to decline in number since its formation in the late 1960s (following inauguration of *Journal of Mathematical Psychology* in 1964). We will see that there is widespread anxiety about quantitative training in psychology, not simply inclusive of mathematical psychology per se. We can question ‘why’ and ask about relationships to various movements in psychology and cognate areas.

2.10. Fractionation vs. generality

One trend over the past century up to the present has been the steady fractionation of science in general and certainly psychology in particular. This trend has been associated with many benefits, such as deepening knowledge and specialization within many branches of study. Yet, it has arguably, if rather paradoxically, been at the expense of areas like mathematical psychology which seek to encompass a wide sweep of individual domains, from learning and memory, to sensation and psychophysics (indeed to at least one society for each modality!), to models of social behavior, to neural theory. As these specialties have budded and flowered, generalistic groups such as SMP have often experienced hurdles in capturing time from over-extended researchers.

2.11. Is psychology now ‘sold’ on mathematical psychology, having effectively absorbed it?

One argument heard occasionally, even by ‘friends’ of mathematical psychology, is that now quantitative modeling has been absorbed by the field at large and hence a separate sub-discipline devoted to this specialty is superfluous. For instance, serious obstacles in the path of publishing mathematical models encouraged the establishment of *Journal of Mathematical Psychology*. Supporting evidence is proffered that many experimental journals now regularly accept quantitative modeling of the experimental data within submitted articles. I agree that this is definite evidence of progress. Yet, the acknowledgement of the fact does not lead inexorably to the consequent. For one thing, journal editors reveal an astonishingly high variance with regard to their attitudes toward quantitative modeling. And, many if not most, outside of quantitative journals, pre-form some type of upper-bound on the degree of abstraction they deem acceptable.

My view is that import alone should determine what sees the light of day even in our more experimental or qualitative organs, but with the stipulation that the editor should be free to request fairly considerable clarification and even tutorial material. A rebuttal might be to the effect that “Well that just means more publications for the *Journal of Mathematical Psychology*”. A problem with this reasoning is that if there is little or no overlap with the experimental journals, it is even easier for less quantitative scientists to ignore our work.

I think it can be fairly argued that SMP and *Journal of Mathematical Psychology*, in addition to sister organizations and journals such as Psychometric Society and *Psychometrika*, are greatly needed by scientific psychology as “keepers of the flame”. We serve as upholders of the highest quantitative standards by our multitudinous duties as reviewers of papers and grants and as practitioners of modeling and methodological science. All this, in addition to our function in training of graduate and undergraduate psychology majors.

2.12. Undergraduate training: Following the trends in society

Many surveys and studies have documented the steep decline of scientific training and acumen in our youth, not to mention the deterioration of scholarship in general (e.g., witness the lamentable pruning of elementary and high school courses in music, foreign languages, and even physical education, starting in the 1970s). Hence, it should come as no surprise to learn that scientific education of US college students is woefully inadequate.

With the possible exception of some well endowed private universities and colleges, psychology departments serve as bountiful cash cows for their institutions, even (or especially) in large, public research-oriented universities. In addition to sizeable flocks of majors plus non-majors, they typically bring in large amounts of outside research monies, sometimes close to or exceeding the more established laboratory science departments. And, their classroom lab facilities cost little to nothing in comparison with the latter.

The debt to the devil in all of this is that there is immense pressure, if usually implicit, against driving down enrollments by increasing standards, for instance, by requiring majors to take more physical and biological science and mathematics. Of course, this influence sums with many others, including the aversion certain sectors and individuals within psychology feel towards mathematics and hard science.⁸ Other pernicious forces include the seemingly perpetual inclination of publishers to persuade authors to ‘dumb down’ their textbooks and sometimes, even scientific monographs, and the well-documented grade inflation that has plagued higher education at least since the advent of the 1970s.

With regard to undergraduate training, given the above and other factors, expecting a sea change toward solid-science education, even for most psychology majors, not to mention the legions from other departments who take our courses, is akin to belief in the tooth fairy. The only practical solution I can espise is for psychology departments to offer a true scientific psychology track, with mandatory courses in the sciences, mathematics and statistics. It could, but need not, be incorporated into a true honors program.⁹

The latter could include options for coursework in engineering, economics, ecological sciences, and more recently available, informatics and biocomplexity, which would increase the chances of those who decide not to pursue postgraduate education, to find employment. An added benefit to psychology graduate programs throughout the land, would be a diminution in the usual scenario every spring: thirty or so departments fighting over a pitifully small handful of qualified students, the latter of which either come from other sciences or somehow manage to acquire decent background in the face of feeble departmental curricula and inadequate counseling.¹⁰ Certainly, the uninterrupted flow of milk

⁸ Naturally, much of this aversion is muted, especially overt statements of opposition to “hard science” in general. However, one could not get far, even in rigorous neuroscience, in the absence of a modicum of real mathematics (e.g., the calculus). In addition, shocking as it may seem, occasionally first-hand anecdotes surface from individuals in major universities, of prominent research psychologists disparaging quantitative methodology and training (and this includes psychometrics and statistical methodology; not just mathematical modeling).

⁹ It has appeared to me that many departments and universities began to downgrade the standards associated with honors programs back in the 1960s and 70s. The reasons may vary but at least in some cases, one motive was to avoid damage to self esteem—a noble goal, but perhaps at odds with the definition of “honors”.

¹⁰ Who among us has not heard some version of the following undergraduate’s refrain (and for me across several universities where I’ve taught): “But the counselor laughed when I asked about taking math and science, and asked why a psychologist would need something like that”. Of course, the one saving grace has been the relatively continuous influx of quantitatively prepared students from abroad. However, even this palliative may fade as countries begin to establish their own fine universities and research institutes.

from the contented cow should gladden the hearts of deans and presidents.

2.13. Graduate recruitment and training

Given this prelude, perhaps it should not come as a surprise that even graduate training in psychology has seen the devolution of quantitative training; never that auspicious in the first place. Several prominent psychometricians reported and discussed a survey on quantitative training of psychologists in 1990 (Aiken, West, Sechrest, and Reno (1990); cf. Townsend (1994)). At that time outdated topics were a major concern with a consequent under representation of newer, powerful strategies.

Fifteen years later we have a reprise on a considerably more frightening note, in an article by Clay (2005) written for the APA Monitor: the shocking scarcity of new quantitatively-trained psychologists, and even of qualified programs to train them.¹¹ Stephen West (Editor of *Psychological Methods* and Professor at Arizona State University) is quoted “At lot of the major quantitative programs over the years have died. We’re one of three larger programs in psychology in the country, and we produced one Ph.D. this year”.

Although mathematical psychology per se, probably never saw more than three or four formal programs of training in the US, it is still arguable that even that limited presence has declined. Of course, the dearth of rigorously trained post-baccalaureate psychologists mentioned above, and the degradation of science in general in the US, are undoubtedly contributing factors.

Now, the Clay article portrays the view from some methodologists that quantitative psychologists are in great demand, with some institutions finally ‘giving up’ when years go by without a successful quantitative hire. That may be and if so, I applaud that there is now a palpable appreciation of our specialty, without, of course, hearty clapping for the diminution in numbers. I must say though, that in my opinion, years went by where superbly trained mathematical and psychometric psychologists were not accorded the best job opportunities. Too often, the unspoken refrain seemed to be something of the form “...well, everyone learns statistics at a sufficient level to teach our introductory courses [often the only kind offered], so we might as well hire in one of our favorite content areas and be doubly happy ...”. And, a rather dangerous pitfall often awaited young quantitative psychologists, that of being expected to provide ‘free’ consulting services to faculty and students (who often didn’t bother to take the assistant professor’s classes on the topic), yet receiving little credit for these activities at tenure and promotion time.

As other possible aversions to the beginning graduate student, in addition to the sheer arduous technical preparation (years of mathematics plus the applied quantitative tools), it has appeared

¹¹ Partly to lodge support for this Monitor article and partly to stress the contributions of mathematical psychology to quantitative training in psychology, I co-authored with Richard Golden and Thomas Wallsten, a guest editorial on this issue for the APA Science Directorate (Townsend, Golden, & Wallsten, 2005). The topic of quantitative training in psychology was raised several times in various ‘break-out’ sessions at the recent APA sponsored Science Leadership Conference. Admittedly, APA has in the past been accused of short-changing the ‘science’ in favor of professional concerns, undoubtedly one of the several motivations for the establishment of American Psychological Society. Nonetheless, there is some evidence that APA, in addition to its decided interest in professional matters, is credibly moving into the game of promoting scientific psychology. For instance, I was recently able to recommend ‘challenges in quantitative training in psychology’ as a theme for the next leadership conference. Steve Breckler, Executive Director for Science, APA, appears to provide a healthy force in this direction (see Psychological Science Agenda (see psaA@APA.ORG, for more on APA’s role in psychological science). In fact, APA is in the process of forming a task force on quantitative training in psychology and hopefully such movements can help turn the tide.

to me that it is the very rare neophyte (i.e., newly minted quantitative assistant professor) who can publish at the rate easily achieved by their cohort in other specialties of the field. Some departments, T&P committees and significant individuals (especially pertinent is the chair or department head) do appear to ‘handicap’ according to sub-discipline when considering promotions and salaries, but I think this is not common and when done, probably not to the appropriate degree.

Why, it might be countered, does the burgeoning field of cognitive neuroscience seem to have little difficulty in attracting apprentices? It is true that a huge range of technical ability and background is accommodated within that discipline but the same could be true in quantitative psychology. More persuasively, neurophysiology has been a more entrenched part of psychology – indeed, almost every department has an ‘area’ devoted to neuroscience or neurocognition (the “in” terms for this region, ‘physiological psychology’, ‘psychobiology’, and so on, seem to change every decade or so) – since the very inception of scientific psychology. There are also feeder sources and ancillary training posts for the physiologically inclined, like pre-med and biology for which there is little concomitant in our area. Interestingly, the current movement toward neuroscience forms one of the few trends in psychology toward hard science.

In any event, it can be cogently argued that the central advantage psychology has over other fields in years past has been the relatively heavy component of education in practical statistics and methodology and potentially, modeling. Hundreds of hours of arduous, often boring and occasionally exciting, labor in the laboratory plus the subsequent data analysis and model testing, put psychologists in a solid position not only for academic positions but also research and management in industry and government.

The pivotal role of experimental and methodological psychologists in effectively leading medical research teams especially in experimental design and data analysis, while typically serving under the obligatory M.D. principal investigator, is well known. And, clinical psychologists have neither the political clout, M.D. prestige, nor monetary recompense afforded psychiatrists. But, they have in the past been able to contribute their knowledge and practice of test theory and administration. These skills largely disappear in the unfortunate trend toward so-called Psy.D. degrees, which require little if any research experience, statistical knowledge or even training in psychological test theory. However, there are now forty-five member training institutions in clinical science, which adhere to the principles of the Academy of Psychological Clinical Science. The goal of these programs is to emphasize the rigorous training of a core of clinical scientists (see, e.g., McFall (2006)) perhaps somewhat compensating for an avalanche of Psy.D. practitioners over the past couple of decades. I don’t know if research has been accomplished regarding the relative proficiency in therapy of Psy.D. personnel vs. traditional Ph.D. clinical psychologists, but there is no doubt about their respective statistical and research skills. Although as one would expect, one of the major regions of cross disciplinary training for clinicians is in neuro-science, some departments, such as that at Indiana University, train some students in mathematical modeling and collaborate with the quantitatively oriented faculty.

Many of us have witnessed even people with Ph.D.s in such fields as physics, engineering, computer science, and mathematics, making grave errors in experimental design when they ‘cross-over’ into experimental psychology in the absence of collaboration within the latter.¹² The same is true, and more, with regard to

¹² This is not a criticism of these groups. Who would expect even a superbly trained mathematical psychologist to do the work of a physicist, chemist, or engineer?

competition of our clinicians with psychiatrists in the ability to organize, run and analyze research programs, and of course, to give and interpret psychologically based assessments. In addition, at present, unless they also have had significant training in experimental psychology, especially in laboratory work, they often seem to be deficient in the 'lore' of what solid psychological research is all about.

Will mathematical psychology perish? In fact, will psychology as constituting the leading body of investigators into psychological research issues, shrivel and fade? Unfortunately, I think neither of these grim results can be ruled out. As already noted, outstanding scientists from the disciplines mentioned earlier, are beginning to attack research problems in regions formerly thought to be our province, in numbers almost meriting the term "flood". No fools they, it is actually surprising that we've had the field to ourselves so long. They collectively bring an armamentarium of research tools that we could well wish our own matriculating psychologists were mastering.

Naturally, it follows that the science itself will not die, not even quantitatively oriented theory and theorists. Only it would be a little bit sad if psychology as a formal discipline, not to mention our own mathematical psychology, both with their venerable history, should fail to continue as the core of mental science.

Let us turn finally turn to what I hope are some further interesting challenges as well as actions in which we can engage that can lead to a more promising tomorrow.

3. Denouement of the present: The future

What are some hurdles that must (or at least should) be overcome in order to optimize our science? What can we do to remain vibrant and even grow over, say, the next fifty years? These two goals may be thoroughly intertwined. Some possibilities that occur to me follow below. Some are continuances of pathways already opened and no doubt there are many more that will be offered and implemented in the time to come.¹³

1. First, I make mention of three barriers in modeling: A. The challenge of distinct theories which predict the same major data corpora, and even become more structurally alike as they strive to encompass new and old data. This barrier is often found when a subject matter has been probed by models and experimentation over a number of years. A classic example, even within the sphere of 'less mathematically precise' is found in the comparison of Tolman's quite centralistic theory of behavior vs. Hull's more behavioristic (both, of course, became known as "neo-behaviorists", and we hasten to reiterate that Hull made more effort at mathematization of his concepts). This may be happening within certain well-studied areas such as long-term memory (doubtlessly, there will be some disagreement here) and perhaps to some extent, within areas of the categorization literature. Of course, new data and experiments can continue to yield fruit, but there is a danger of new studies becoming increasingly 'precious' as to the decreasing consequence of the new phenomena. One way to break this cycle may be the import of neuro-imaging and other physiologically based strategies. B. The challenge of model complexity. Ever since the dawn of cognitive science, there has been the obstacle posed by extremely complex models. Jokes have often been made to the effect that one might as

well be studying the human brain as opposed to trying to figure out what a very complicated model is predicting, or especially *how* it is predicting it. Although this problem originally reared up within 'traditional' AI type models (e.g., those based on seemingly interminable computer programs in LISP), it reappeared in the embryonic connectionism. Even relatively simple models with hidden units could be rather inscrutable. Modern connectionist models tend to take several tacks in negotiating these 'rapids': a. Hidden layers are assigned specific roles to play in a cognitive task. b. Naturally, the tried-and-(maybe) true usual statistical techniques such as factor analysis, etc. are brought to bear on the black box system. c. Theoretical lesions (a non-black box approach) are employed to discern the various mechanisms. It has also been claimed that so-called 'local connectionist' models are less prone to this vein of identifiability problem than 'globally distributed' models (e.g., see interesting discussions in Grainger and Jacobs (1998)).¹⁴ C. Even rather simple experiments can require more data than is reasonable to acquire. For instance, general recognition analysis of various types of independence works optimally when a factorial design is employed with a 1–1 stimulus–response assignment. Thus, a five dimensional experiment with four levels on each dimension would demand 4⁵ or 1024 stimuli. Only 100 trials per condition (we like to run at least 300/condition), precipitates over one hundred thousand trials. At 500 trials per day (quite an arduous schedule), this would consume approximately ten months. There had better be a darn good chance of accumulating solid meaningful data, for a very good theoretical reason, to justify such an experiment. Of course, the time honored tack here is to run multiple subjects. This would still be in most cases, unpractical to say the least. More theoretically problematic is the traditional practice of subject averaging, now acknowledged almost universally to be flawed not only in process modeling (e.g., Ashby, Maddox, and Lee (1994) and Estes (1956)) but also in psychometrics, where it is omnipresent (see Molenaar (2004)). Nonetheless, obeying the precept that (almost) nothing is categorical, we have some 'just in' findings which indicate that for small data samples, fitting models to subject averages across is sometimes superior to individual fits (Cohen, Sanborn, & Shiffrin, *in press*).

2. Mathematical psychologists and psychometricians should join forces to strengthen and broaden quantitative training of psychologists. Perhaps for historical reasons, there seems to have been reluctance by parties of both sides to do much of this in the past. Inexplicably, commissions on quantitative training in psychology have sometimes omitted to involve representatives of mathematical psychology. Perhaps the present crisis will encourage more cooperation (even if of the form "If we don't all hang together, we shall certainly all hang separately").
3. We should encourage the construction and strengthening of undergraduate tracks which possess higher standards and upgraded, more scientifically oriented and quantitatively dependent, class material. Even though these would probably be expressed as something of the form, "...for honors-level psychology majors..." they would be open to capable students from mathematics, as well as the physical and biological sciences.
4. Psychology should require more undergraduate and graduate courses in mathematics and the physical sciences. Perhaps psychology departments should offer more elementary mathematics courses integrated into psychological statistics and

¹³ Naturally, we hereby enter irrevocably the portals of "do as I say not as I do", since I could well have toiled more assiduously myself on many of these paths. Nonetheless, hopefully even if most of us attempt to 'pay our due' in a small sector of these, the cumulative effect will be mighty.

¹⁴ I'm indebted to John Kruschke for an enlightening discussion of these matters.

modeling in the same spirit as do the various fields of engineering. Outside of the usual very low level statistics courses required of everyone, these might be reserved for the undergraduate students meeting the standards mentioned in (2) above. Although we will never stand independent of courses from mathematics, building more of our own courses and requiring at least some of them possesses a number of advantages. These include: A. Uniform training in various topics. B. Affords an opportunity to employ examples from the social and biological sciences, especially scientific psychology. This tactic renders the material more interesting and convincing and helps lead into the applications in advanced psychology courses. C. Instills an esprit de corps (admittedly perhaps somewhat like the “we’re all in boot camp together” phenomenon) which helps bond budding psychologists. D. Acting as ‘feeder courses’ into more advanced quantitative methodology and modeling training.

Of course, we face not only opposition from certain sectors of our own discipline but the usual territoriality of other departments which teach similar material (again, engineering has surmounted this common challenge and, presumably, so could we). Another challenge, maybe especially pervasive in graduate echelons, is the often oppressive number of general core courses required of graduate students. However, this type of challenge is somehow circumvented by psychological neuroscientists so I don’t see it as insurmountable.

5. My opinion is that we have to be willing to teach statistical and general methodological courses within our own department.¹⁵ It is a lamentable fact that there is significant pressure to teach these at what we consider to be a criminally low level. However, in most college and university settings, the number of ‘slots’ accorded any sub-discipline will be at least partly, and sometimes completely, a function of service teaching responsibilities. We will almost certainly wither as a field if we cannot justify our contributions partly through teaching commitments.
6. Our thinking on the exploding interest by applied physicists, computer scientists, applied mathematicians must be diverted from “here come the (perhaps ‘at least as smart as us’) barbarians”, to a more positive outlook.¹⁶ We should try to bring them into collaborations and perhaps mutual teaching assignments, perhaps in advanced courses or seminars. We could encourage the most interested to attend SMP, and other scientific psychology conferences. Our students attend their courses, so perhaps we could lure some of their students to take ours. The burgeoning field of cognitive science and related disciplines, both in academia and industry, certainly offer attractive job prospects for some excellently trained scientists (e.g., high-energy physics and mathematics) whose job prospects may not be too rosy these days, unfortunately. And, it would jointly aid them and our science, not to mention quantitatively sound research, if they were to receive more training in our content research areas.
7. Related to but not identical to (5), practitioners of the new fields of brain imaging are hungry for tools and training pertaining to methodology and data analysis of their data. We could make a real contribution to their statistics

(e.g., time series; parameter estimation; hypothesis testing) and engineering (e.g., systems identification) approaches to name a couple. In addition, this is one of a number of areas where statistics and substantive process modeling could synthesize with the neuroscience to offer vigorous instruments for progress. Especially rich opportunities lie in the relative scarcity of means of comparing and provisionally linking two or more types of data, for instance, behavior, fMRI, EEG, PET, single unit recordings, and so on. Ashby’s (e.g., Ashby, Alfonso-Reese, Turken, and Waldron (1998)) recent modeling ventures into such regions appear prophetic. These movements are going to be of enormous consequence in the years to come.¹⁷

8. There is a small, but significant and hopefully growing presence of rigorous process modeling in clinical psychological science. One of the earliest pioneers has been Richard W.J. Neufeld (e.g., see his new edited volume, (Neufeld, 2007); papers include Neufeld (1993) and Neufeld, Vollick, Carter, Boksmann, and Jette (2002). Other innovative examples include Stout, Rock, Campbell, Busemeyer, and Finn (2005) and Treat, McFall, Viken, and Kruschke (2001). In point of fact, there is a sector of clinical science where the researchers are as ‘tough-minded’ as any in cognitive science, as and perhaps more open to mathematical modeling than some of the latter. It would behoove us to welcome and offer our ‘aid and abetting’ to this small but growing field.
9. Simple finite-state process models, most often with a Markov assumption transiting between states, have been with us for quite a spell (e.g., finite-state signal detection models mentioned earlier). Batchelder has assembled a very general theory-driven methodology and related statistical armamentarium based on this concept, which he terms *cognitive psychometrics*. These constituent models differ from traditional linear and log-linear models by virtue of their tree structure.
10. In addition to mathematical psychology’s time honored ability to test parameterized models against experimental data, I won’t miss a chance to plug approaches that test entire classes of models against one another in ways that are invariant over specific distributions and parameterizations. Such theory-driven methodologies (I like the term ‘meta-theory’ or ‘meta-modeling’ for such approaches), have been especially prominent and successful in identification of mental architecture and accordant mechanisms in response times (e.g., Schweickert, Giorgini, and Dzhamfarov (2000) and Townsend and Wenger (2004)) and featural and dimensional independence in accuracy (e.g., Ashby and Townsend (1986) and Kadlec and Townsend (1992)).
11. In the 1970s and beyond, mathematical psychology finally began to build models capable of handling both accuracy as well as response times (e.g., Link and Heath (1975) and Ratcliff (1978)). Yet, for decades our models and most of our data have been relatively confined to $n = 2$ stimuli and responses. It is important both for the basic science as well as applications in many fields to extend our theories and methodologies to sizeable values of n for stimuli and responses.
12. In the last decade or so, a number of mathematical psychologists have played major roles in plowing virgin territory in model testing (e.g., see the special recent JMP issue on model selection guest edited by Wagenmakers and Waldorp (2006) and the slightly older JMP issue edited by Myung et al. (2000).

¹⁵ Not everyone in quantitative psychology agrees with this declaration, as witnessed by several conversations I’ve had with colleagues recently at conferences; thus the ‘opinion’ clause.

¹⁶ Intriguingly, not all of this interest in deriving from what in the past have been called “applied” mathematics. A few years ago, I attended a workshop at a respected mathematical society on the topic of learning machines which included presentations by several topologists, not often thought of, even by other mathematicians, as especially ‘applied’.

¹⁷ Speaking personally, I have been ‘having a ball’ in the last couple of years, now that we have a T-3 fMRI machine at IU, interacting not only with neuroscientists, but also with the physicists and others either helping run and maintain or simply being attracted to this new technology.

Some of this work, pioneered in psychology by Myung and colleagues, exploits novel concepts from complexity theory and permits for the first time, model testing to take into account the ability of a model to account for a wide range of data, apart from the sheer number of parameters. These new vistas can not only aid our own field, but help us to aid colleagues from other fields, such as mentioned in (6) just above.

In closing, I suppose it's considered rather outré' to sound like a cheerleader or NFL coach, but I do hope that each of us can do something, in addition to our personal research, to make a contribution to our field. Mathematical psychology has arguably accelerated the evolution of psychology and allied disciplines into rigorous sciences many times over their likely progress in its absence. Let's nurture and strengthen it.

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